

(d.) Elongation accompanying rises of internal pressure.

(e.) Pulsatile increase of volume during sudden brief elevations of internal pressure.

The discordant results obtained by various observers (Marey, Roy, and others) as regards the distensibility of arteries, are in large measure explicable by the absence or presence (in varying degree) of *post-mortem* contraction in the arteries they used in their experiments.

“Heredity, Differentiation, and other Conceptions of Biology : a Consideration of Professor Karl Pearson’s paper ‘On the Principle of Homotyposis.’ ” By W. BATESON, M.A., F.R.S.
Received January 25,—Read February 14, 1901.

In his paper on “Homotyposis,”* of which an abstract appeared in the ‘Roy. Soc. Proc.,’ vol. 68, p. 1, Professor Pearson raises an issue of extraordinary importance. In any attempt to perceive the true relation of variation to differentiation, and to analyse the essential similitude existing between Heredity and Repetition of Parts, we reach a fundamental problem of biology. Little has thus far been done towards elucidating this problem or even towards formulating it. The appearance of Professor Pearson’s remarkable memoir may perhaps therefore with profit be taken as an occasion for considering critically some aspects of these questions.

It is impossible to write of Professor Pearson’s paper without expressing a sense of the extraordinary effort which has gone to its production and of the ingenuity it displays. But on careful examination it will, I think, be seen that in the light of known facts there is serious doubt whether the determination of what Professor Pearson calls the average homotyposis of “undifferentiated like parts” can be attained by his observations, and that there is even graver doubt whether, if it was attainable, such a value would have any natural significance. In the course of this consideration it must, I think, also appear that the comparison he attempts between the average homotyposis of “undifferentiated like parts” and average fraternal correlation in families is incorrectly instituted.

At the outset I wish to express the conviction that the leading idea which inspired and runs through the work is a true one. Professor Pearson suggests that the relationship and likeness between two brothers is an expression of the same phenomenon as the relationship and likeness between two leaves on the same tree, between the scales on a moth’s wing, the petals of a flower, and between repeated parts

* ‘Phil. Trans.,’ A, 1901, vol. 197, p. 285.

generally. The conception of heredity is thus greatly simplified, and that phenomenon is seen in its true relation to the other phenomena of life, becoming merely a special case of the phenomenon of Division and repetition of parts.

This idea came first to me—as it has perhaps to others—when I was studying the phenomena of Variation in Meristic Series, and in writing on that subject I introduced an outline of the conceptions involved.* On that occasion I ventured to carry this reasoning a step further, as it seemed, and to suggest that *the resemblance which we call Heredity may be a special case of the phenomenon of Symmetry*. The thought then expressed has been a constant companion ever since, and I have become more and more convinced that it is fundamentally true.

I should welcome Professor Pearson's paper inasmuch as it is an attempt—the only one, so far as I know—to emphasise and develop this conception; for, like him, I am sure that it may provide the key to the nature of heredity, perhaps also to problems beyond.

Variation in some of its essential features may thus perhaps be reduced to a geometrical problem. One of the many factors or conditions of fraternal resemblance may be Symmetry of division, quantitative and qualitative. The reference to the phenomenon of Symmetry seemed to me to carry the principle a stage further, and to show Heredity as a special case of a phenomenon, the conditions of which we may reasonably hope, in a measure, to apprehend in at least its simpler phases. Professor Pearson, on the contrary, avoids mention of Symmetry. This arises, I presume, from a desire to use a more general expression, and from a reluctance to appear to exclude from his comparison the relation between members of Linear or Successive Series, whose mutual relationship is not one of Symmetry in the ordinary sense. He would probably prefer to regard Symmetrical division as a phase or perhaps as a consequence of the phenomenon of the production of “undifferentiated like parts” occurring under special conditions.

I still think something is gained by inverting the statement and speaking of the likeness between the parts as a phenomenon of Symmetry. In some respects Professor Pearson's mode of expression is preferable as being more comprehensive, but mine has the advantage of keeping before the mind the fact that it is in the *Symmetry of cell-division* that the *resemblance* between relatives is presented in its simplest form; and also that the axes along which the “like parts” are produced are frequently definite.

Thus, fraternal correlation in its most striking manifestation is seen in the simultaneous variations of Homologous Twins.†

* For a somewhat fuller treatment see ‘Materials for the Study of Variation,’ 1894, Introd. Sect. VII; and also p. 21.

† See ‘Materials,’ pp. 559 and 560. Following the work of Driesch and others

It is true that the geometrical relations of members in Successive Series are not included in the term Symmetry,* but the distinction is largely one of degree, and the transition from one to the other is of frequent occurrence. Similar organs may be repeated in one species in radial series, while in an allied species the same organs by differentiation of an axis stand in succession to each other, as every naturalist knows.

Professor Pearson writes (p. 291): "When we ascertain the sources of variation in the individual, then we shall have light on the problem of fraternal resemblance." May we not also say that when we ascertain the conditions of asymmetrical division we shall have light on the problem of fraternal variation?

I introduce this reference to my own method of expression partly to show how far I am in agreement with Professor Pearson on a main point, and partly to emphasise the significance which the analogy between Repetition and reproduction gains by the reference to Symmetry.

Theoretical considerations in mathematical form are put forward by Professor Pearson as tending to the belief that the numerical value for homotypic correlation will, on an average of cases, approximate to the average value found for fraternal correlation.

The reasoning is beyond me, but I gather that the argument, by the introduction of appropriate assumptions, amounts to a proof that if the characters of the offspring, as measured by their deviations, depend on those of the germ-cells of the parents, then the characters of the repeated parts (or *undifferentiated like* parts) formed by that offspring will similarly depend on those of the germ-cells; and it would then be expected that the correlation between those repeated parts of the same individual would be similar in intensity to that between the germ-cells of its parents. Whether the assumptions are justifiable I am not able to judge, as I do not properly understand them.

The resemblance or correlation between "undifferentiated like parts" is, then, regarded as a phenomenon similar to the correlation between brothers. The latter correlation has been investigated by Professor Pearson in a number of heterogeneous cases, and has been found to vary from .1973 to .6934, where 0 is zero and 1 denotes complete correlation.† The mean value approaches .45. He pro-

on the artificial production of Double Monsters, we must regard the relation between Homologous Twins as of the same nature as that subsisting between the right and left halves of a bilateral organism.

* It would be easy to suggest terms better adapted to the expression of these conceptions, but to do this at present is premature. When it becomes necessary to do so I anticipate that the largely analogous phenomena of rhythmical vibration will provide ready metaphors from which to construct a terminology well adapted to denote the various phenomena of Merism.

† Both here and in the coefficients of "homotyposis" reasons are given for supposing that some of the greater departures from the mean may be explained away.

poses in this paper to find a numerical value for the average correlation between undifferentiated like parts of the same individual. A large series of heterogeneous cases of various organs in various plants have been investigated. The values found range from ·1733 to ·8607. Reasons are put forward for excluding some of the highest and for doubting the validity of others, especially some of the lower ones. Eventually the average result ·45 is again reached, taken on a series ranging from ·1733 to ·6311.

Professor Pearson attaches importance to the rather close similarity between the two average values. We are bound, therefore, to remark as a suspicious circumstance that the range of values is so wide, and that the average value should so nearly approach the mean of the whole possible range; but upon this point I do not propose to dwell, preferring to deal with more general aspects of the problem.

Now Professor Pearson is attempting to measure to what extent there is a resemblance or correlation between repeated parts of one individual as compared with the same parts of different individuals of the same race: how much, that is, of the resemblance between repeated parts of an individual is due to its individuality? Further, how much on an average of many individuals may be expected to be due to individuality?

For various sources of error Professor Pearson is well prepared. In his *Malva* material, for instance, he finds little correlation due to individuality; because, as he points out, his specimens may have been all or largely the vegetative product of one or few individuals. In some Mushrooms, on the contrary, he finds this correlation high, but he thinks that here individuality may partly be due to stages of growth, for his individuals were not all of similar age. In comparison with what follows these sources of error are trifling.

It will be remembered that the series of homotypes is to be *undifferentiated*. If differentiation exists and is not recognised the apparent homotyposis due to individuality will, as Professor Pearson perceives, be immediately lowered. In order, therefore, that the inquiry should have significance, it is necessary that differentiation occurring between members of a series of parts should have a clear meaning as distinct from variation occurring amongst them; and further, in order that the investigation should be carried through, we must be able to discriminate such differentiation from variation. On critical consideration it will be apparent that neither of these postulates accords with the facts of nature. I cannot find that Professor Pearson has in any real way dealt with this difficulty. The practical difficulty he has perceived and in part met, but the more serious theoretical difficulty has, I think, escaped him. When fully understood, it will surely be seen to invalidate the whole argument.

Let us grant for the moment that differentiation of the parts can be

dealt with—if it can be detected; but if differentiation occurs in different individuals in different degrees and directions, how can it be told whether the ensuing deviations in correlation are due to a change in the control of individuality over the variation, or to irregular and incipient differentiation? Yet, is not such differentiation exactly what is to be expected in the variation of homotypes? Do not most animals and plants exhibit this phenomenon, and must we not believe that these organisms have attained their present forms largely by variations among their repeated parts? In view of these familiar facts, can Professor Pearson point to any feature which positively distinguishes variation occurring between members of a series from differentiation?

That differentiation may in practice be mistaken for variation between homotypes he is aware. It is not, however, the difficulty of recognition I would now emphasise, but the fact that between the two phenomena no absolute distinction exists in nature. An “undifferentiated series of like parts” means only a series of like parts which have varied and are varying among themselves but little. A series of highly variable like parts is a series in which differentiation exists or is beginning to exist in a complex and irregular fashion. A “differentiated series of like parts” means a series among which variation is or has become definite and regular. Between these classes there is every shade and degree. No one can say finally where each begins and ends, and, by appropriate selection, we could find homotypic coefficients of any required value. The *average* value of such coefficients taken at random has no significance in nature.

Let us examine some practical examples.

In Professor Pearson's *Nigella*, for example, the correlation between the numbers of segments in the capsules of individual plants is found to be low. That is to say, given one seed-vessel of the plant, it will give you very little information as to the most probable number of segments in a second seed-vessel of the same plant. Why is this?

From the look of the plant, or, if such simple perceptions are mistrusted, by counting the segments of seed-vessels on lateral branches, and comparing the numbers obtained with those obtained from seed-vessels borne on central axes only, it is easy to show, as Professor Pearson points out, that the numbers are generally lower in the case of the laterals. We recognise, further, that the proportion of laterals varies from plant to plant.

How is the differentiation detected in *Nigella*? By the *regularity* with which small capsules are associated with lateral branches.

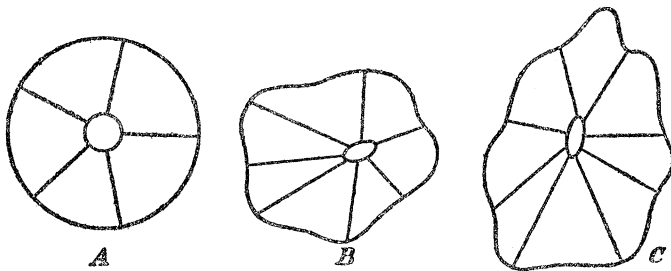
But suppose that for any reason this regularity were masked, should we then perceive the differentiation? Might it not pass, wholly unsuspected, for a change in correlation? Undoubtedly it might.

Take the case of blood-corpuscles of a Frog. Measure some charac-

ter of a hundred corpuscles in each of many Frogs. Find the correlation due to the individuality of the Frog. How can we determine whether in some of the individuals we have used there may not be differentiation such as was found in *Nigella*, so that the parts are not really "undifferentiated like parts"? Would not such irregular differentiation change the mean correlation between the corpuscles?

Would not the fear of such an error practically lead to the exclusion of cases of suspected differentiation *which ought to be included* in calculating the average? Is it not certain that differentiation in important characters may take place in exactly the masked way I have referred to? If, for instance, we could count granules in the corpuscles and work out their homotypic correlation for these numbers, might not we have among our individuals some which had specialised corpuscles absent in others?

Again, does not individuality show itself by *change in the degree* of differentiation among homotypes? Can we frame a definition of variation which will exclude such changes?



A represents a radially symmetrical organism in which we may study the correlation in lengths of the radial septa and determine how much is individual or homotypic, and how much racial. If the radial symmetry were always perfect and the specimens merely of different sizes, the racial and homotypic correlations would be alike, unity. But suppose the population consists partly of (i) approximately radially symmetrical specimens; (ii) quite irregular specimens like B; (iii) of specimens whose forms are controlled by an incipient differentiation of any axis tending towards such a form as C.* How would Professor Pearson's methods determine the true homotypic correlation in this population?

Suppose that in a polychaet, say a Syllid, there is marked differentiation between segments at the anterior and posterior ends separated by hundreds of segments apparently undifferentiated, bearing appendages similarly undifferentiated. We may determine the homotypic

* I have no doubt that a study of the Corals, say, would provide actual examples of such a population. May not some Mushrooms be in just this state?

correlation of these appendages. So long as differentiation regularly and conspicuously begins at a certain region we can exclude it. But suppose in some individuals it begins at one segment and in another at another, as it almost certainly would do, how should we know which specimens or which segments must be rejected as introducing a confusion through differentiation, and which must be included in reckoning homotypic variation? If differentiation is irregular, will it not change the apparent homotypic correlation?

Professor Pearson has determined the racial correlation for the lengths of the first phalanges of digits III and IV in women. It comes out high, .914,* as might fairly be expected by any one who had studied the meristic variations of digits. There is, of course, differentiation between these two digits, so that they may be said to be unsuitable subjects for determination of homotypic correlation of like parts. But if instead of Man, the digits III and IV had been studied in an Artiodactyle, say a Deer, the *racial* correlation would doubtless have been much nearer unity. In other words, these two digits in the Deer are approximately in the relation of bilateral symmetry about the median axis of the foot.

In this case the differentiation between the digits is low. They approach the homotypic condition, and their homotyposis could be measured. But a population may consist of some individuals in which there is a high correlation between these two digits III and IV, and others in which differentiation had begun or sensibly persisted. In such a population the racial correlation would be clearly reduced. But would not the homotypic correlation, as calculated, be changed also? Would Professor Pearson's method show to what extent incipient differentiation had introduced error in the determination of the homotypic correlation?†

Yet another and even clearer illustration. The two claws of a Crab are a pair of homotypes. Their homotypic correlation in respect of any character, length for example, might be determined. Now there are species of crab in which the two claws are approximately equal or undifferentiated. On the contrary, in some species the right, in others the left, in others the right or left with varying frequency, is differentiated in size and other characters. Can it be decided in such a case which deviations from, or approaches to, bilateral symmetry are, *as variations*, to be included in a determination of homotypic correlation, and which are to be rejected as due to changes in differentiation?

On this rather wider view of the facts is it not manifest that the

* 'Grammar of Science,' 1900, p. 398.

† If Professor Pearson declares that such differentiation would be "statistically discoverable," he must assume that the differentiation would always affect the same digit in the same direction, an assumption for which I can see no warrant.

distinction rests on fallacy? The reality of the problem as defined by Professor Pearson depends on the assumption that there is an absolute distinction between differentiation and variation among repeated parts, and its solubility depends on the assumption that this distinction can be perceived. The proviso that such a distinction is to be observed stultifies the whole inquiry. So far are we from being able to perceive this distinction, that we may even doubt whether variation among repeated parts *can* take place except as differentiation. If the idea of variation is to be extended to the case of differences between repeated parts it must inevitably include differentiations among them.

But, unless differentiation can always be detected or always reckoned for, the *average* value of the homotyposis coefficient will have no more natural significance than would the average variability of all organisms measured by their "Standard Deviation" from their various means, or the average size of living cells, or the average weight of all ponderable bodies.

I now proceed to a different point, in a sense the converse of the former. Professor Pearson perceives that the correlation between "undifferentiated like parts" has an analogy with the resemblance or correlation between brothers. But does he recognise that variation between brothers is comparable not merely with variation between repeated parts, *but also with differentiation*, and with predominantly orderly variation among such parts? The phenomena in a colonial or social form will clearly illustrate this principle. Ova and spermatozoa may be treated as "undifferentiated like parts" so long as their variations, judged by the resulting offspring, are sensibly irregular. Can we recognise differentiation among them as distinct from variation? Certainly we *sometimes* can. In determining the correlation of consanguinities, the parentage enables us to distinguish the fraternal groups correctly, and consequently a fraternal correlation may be truly determined. *For to do so we are not compelled to distinguish differentiation from variation.*

But I put it that the parallel Professor Pearson is seeking is improperly instituted in his paper. He compares the homotyposis of "undifferentiated like parts" with the correlation between brothers. *He ought to compare it with the correlation between undifferentiated like brothers.* As it is, he is trying to find for homotypes what he would be trying to find if he set about a determination of the average value of fraternal correlation for fraternal groups of *like members taken from families composed of differentiated members.* Such an attempt would immediately necessitate a distinction between differentiation and variation. Had his comparison been correctly instituted, Professor Pearson could hardly have failed to discover the fallacy on which I submit his reasoning is based.

Let me state a case in illustration. In most species of Ants females

are differentiated into workers and queens. Frequently other castes, soldiers and others, are similarly recognisable. As regards formulation of his problem, Professor Pearson will perceive that the parallel to average homotyposis is *not* average fraternal correlation of even all the females from one pair of parents, but the correlation between workers, or between soldiers, &c., of one family. He may reply that this objection, though true on the point of form, can be met by weighting the various castes when they are compared. I doubt whether the difficulty is thus fully met (even if in practice it were possible to carry out the process). Should we even then be comparing comparables?

Should we not still be finding a correlation like that for a miscellany of *differentiated* series of repeated parts?

This reasoning, so clear in the case of Ants, extends to all cases of differentiation between members of confraternities.

To find then a value comparable with the homotyposis of *undifferentiated like* parts we must find the fraternal correlation between *undifferentiated like* brothers. But differentiation has here again no meaning which can be determined with precision. It shades insensibly into variation.

Suppose we merely propose to determine the *average* value of fraternal correlation in *workers* of one genus of Ants. In some species we sort our Ants easily into workers and the rest. In another species we shall find differentiation so imperfect that we cannot say for certain which are soldiers and which workers. Finally, even in the more completely differentiated species we shall find occasional nests (families) which show an imperfect differentiation.* Average fraternal correlation, I think, has no meaning, still less an ascertainable value, in these cases.

The principle that Professor Pearson calls "homotyposis" I have expressed by the statement that the variations of parts, repeated in series, *may* be "similar and simultaneous."† Beyond this we cannot yet go. Professor Pearson's statement of the principle fails to recognise one of the most important features of homotyposis. Expressed in my own terms, Professor Pearson's "homotyposis" is the principle of "similar and simultaneous variation" restricted to *undifferentiated like parts*.

But relationship is not lost when we pass to the differentiated parts, and such differentiated parts *may* vary similarly and simultaneously with other differentiated parts of the same series, exhibiting the phenomenon of Homœosis. A stamen of a Rose, if it becomes petaloid, is not merely a petal, but a *petal of the individual* Rose it is on. Professor Pearson's principle, as stated by him, misses this point.

If he had correctly instituted the comparison between parts and

* See the writings of Forel.

† 'Materials,' p. 569.

individuals he would have seen this also. For in cases of confraternities he must be familiar with the phenomenon of similar variations occurring simultaneously in separate groups of differentiated members.

But let us now suppose we could define differentiation from variation in general, say, as orderly variation. Even so we could not distinguish it unless its order were conspicuous. In a former paper,* Professor Pearson wrote that "the very nature of the distribution of variation, whether healthy or morbid, seems to indicate that we are dealing with the sphere of indefinitely numerous small causes, which in so many other instances has shown itself only amenable to the calculus of chance, and not to any analysis of the individual instance." As I have on many occasions stated, such a description accords ill with the observed facts of variation. Illustrations to the contrary are numerous and are now becoming familiar; and even in Professor Pearson's later works references to them are not wanting.

Does not, then, the presence of orderly differentiation, in various degrees, *compel* us to an analysis of individual instances? In plain language, we shall have to pick and choose our cases, and the value of our coefficient of homotyposis will depend entirely on how we do it. Has not Professor Pearson himself been so compelled in more than one of his examples, notably in that of *Nigella*? Has he any certainty that such an analysis ought not to have been made in other examples also?

He speaks of the extreme difficulty of determining whether his material is homogeneous in respect of environment, but I miss from his work any deep appreciation of the subtle and evasive quality of differentiation. If any one would obtain a conception of this difficulty let him go to any tree or large plant and set about pruning it, or better, let him try to choose shoots for propagation. Until he tries, it seems simple enough; but when he begins he finds the shoots are of many complexly differing kinds, and unless he has experience of pruning or of propagation, he will not know which to choose. If he studies the tree attentively, he will soon see that the kinds of shoots are largely definite and, in fact, differentiated. The differentiation may be irregular or regular. That of the leaves may or may not be correlated with that of the shoots. The differentiations may be correlated with the age of the wood, with the absolute size of the tree, they may be peculiar to the variety, or they may be individual to the specimen and defy analysis.†

I am of course aware that Professor Pearson knows all this, but I

* 'Phil. Trans.,' A, 1896, vol. 187, p. 255.

† There are examples not only of differentiations occurring irregularly in one species and regularly in another, but also of the separation of these very forms of differentiation as characteristics of distinct varieties. See for instance the heterophyllous Junipers and Cyresses.

gather the impression that he regards these differentiations as largely recognisable and capable of exclusion. He may hope too that by increasing the area of his statistics these orderly disturbances may cancel each other. This appears to me highly improbable.

Order in occurrence is generally the only indication of differentiation, and when the order is obscured, differentiation may pass wholly unobserved. But the presence of such differentiation will vitiate the result, even if the area of statistics be indefinitely increased.

The only answer which seems open is that though it may be impossible to define precisely in words which examples should be reckoned in determining average homotypic correlation, and which must be excluded as showing differentiation, yet in practice the difficulty is not a real one, and that divers features (*e.g.*, regularity of occurrence) enable us to detect sensible differentiation.*

Such an answer is far from covering the whole ground of the objections I have indicated, as may readily be seen by attempting to apply it in the practical illustrations given above. But besides this, to take that ground would be to turn back from that appeal to rigid numerical treatment, which Professor Pearson has told us should be the sole test of these hypotheses.

I may further point out that if it were suggested that the distinction between differentiation and variation may be left to the judgment of the observer; we might by a similar exercise of judgment attempt a distinction of variations into evolutionary or *specific*, and *normal*.

This is far more than a merely logical point. I am disposed to think that such a rough classification boldly made and carried out for a number of familiar forms might greatly promote the study of evolution, even though no precise criterion can yet be provided. This suggestion will be abhorrent to many naturalists, though for want of such a distinction much of the statistical work produced by Professor Pearson and his followers has, I believe, gone wide of its mark, if that aim is the elucidation of Evolution. More fitly might this work be described as "Mathematical Contributions to a Theory of Normality."

In the treatises I have referred to Professor Pearson is seeking for a statistical conception of Species through an examination of miscellaneous variations. The impression left on my mind by such imperfect study of his works as I have been able to make, and especially by the present paper, is that the evidence points to some conception of normality to be otherwise attained, a conception more finite and concrete than any we have yet reached.

By the one word *Variation* we are attempting to express a great diversity of phenomena in their essence distinct though merging in-

* Professor Pearson's reference to *Nigella* (p. 320) as unsuited to his purpose because probably "*unstable*" suggests to me he had here this difficulty in view.

sensibly with each other. The attempt to treat or study them as similar is leading to utter confusion in the study of evolution.*

If normality thus imagined can be shown to be a real phenomenon it is conceivable that we might then profitably attempt to determine in specified cases the average value of homotypic correlation for each case, but the average value for a miscellaneous collection of cases would still have no natural significance.

(Note, added November, 1901.)

On p. 287 Professor Pearson has added a note in which he seeks to meet a part of my objections. He says: "A diversity due to differentiation and a variability due to chance are quite distinct things. The one is the result of dominating factors which can be isolated and described; the other of a great number of small factors, varying from organ to organ, and incapable of being defined or specified. Indeed, upon each dominating factor of differentiation is superposed such a chance variability. Of course all things which differ even by chance variation are in a certain sense differentiated." This welcome passage outlines the conception that must form the point of departure in any attempt to understand variation in its relation to Evolution. The same conception I have myself often laboured to express. On former occasions to these two kinds of diversity of which Professor Pearson speaks I have applied the terms "Discontinuous" and "Continuous." Though useful in practice, those terms are open to misconstruction and perversion. In the present paper I have suggested the nearly equivalent terms "Specific" and "Normal." Similarly, to variations occurring among repeated parts or homotypes we might apply the terms "Differentiant" and "Normal." Throughout nature the variations between the members of fraternities may be discontinuous and specific, and in like manner may the variations between repeated parts be specific and differentiant, though in both classes normal or continuous variations are always superposed on them.

In most cases the naturalist is seldom in much doubt with which he is dealing. But though these two great classes of variation can broadly be recognised and treated as distinct, the distinction may be evasive, and when the differentiation is irregular that distinction must often be obscured and not "statistically discoverable." Professor Pearson is mistaken in supposing that such differentiation must show itself in his seriations. It may appear *only* as a lowering

* For example, in his criticisms (p. 360 and elsewhere) of the view that sexually produced offspring are more variable than offspring not sexually produced, Professor Pearson is merely confusing different kinds of variations and applying to certain kinds conclusions derived from a study of another kind.

of correlation. The diversity due to differentiation may exhibit a "homogeneous chance distribution," as, for example, in my illustration of the crab's claws. We have only to suppose that the "mode" of the population falls on a form with claws approximately equal, and—to take the simplest case—that the frequency of both right-handed and left-handed differentiation is inversely proportional to the magnitude of the differentiation, a state of things common enough in nature.

As a matter of fact even in the case of *Nigella* (p. 320) differentiation was detected not by the seriations, but by common observation. When the differentiation has been once detected, its influence can be seen in the seriations. This is a mere accident. If the material had happened to contain a certain proportion of a second race with a "mode" on 10 or 13 and a secondary "mode" on 8—a condition familiar in plants (from F. Ludwig's beautiful researches)—the differentiation might have been completely masked in the seriations.* As it is, the seriations alone contain nothing which *prove* the existence of differentiation. We happen to know otherwise that high numbers are associated with centrals and lower numbers with laterals. This is not revealed by the seriations. For all they show, the irregular distribution might be due to ordinary discontinuous variation obeying the laws which F. Ludwig has shown such distributions commonly obey.

We can feel nothing but admiration for those statistical methods which, as perfected by Professor Pearson, are yielding many useful results not otherwise attainable, yet their limitations must be constantly remembered. But even if the differentiation could be discovered by these means, in eliminating it we should have arbitrarily excluded a class of facts which ought to have been included in calculating *average* homotyposis, or the correlation due on an average of cases to individuality. In determining the *average* correlation between brothers we must bring to account the continuous and the discontinuous alike: so in the *average* of homotypic correlations must be included both the differentiant and the normal alike.

To state the issue in a word: it appears that the attempt to exclude differentiation by definition must constantly fail in practice and is inadmissible in theory.

* I strongly suspect that something of this kind may actually exist in the case of Shirley Poppies.